A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

MARCH 1799.

ARTICLE I.

An Account of Improvements in Electrical Batteries; a Method of augmenting their Power, with Experiments, shewing the proportional Lengths of Wire fused by different Quantities of Electricity, and a Description of a new universal Electrometer. By MR. JOHN CUTHBERTSON *, No. 53, *Poland-street, London*, 1799.

IN the year 1774, Mr. E. Nairne made an Electrical Machine, far superior in acting power to any that had been made before*, and a Battery more judiciously constructed and larger than any former one, with which he made a number of interesting experiments. One in particular, affords an accurate measure of the power of his battery, compared with such batteries as have been made since that time. I mean the experiment of melting or dispersing a metallic wire. His battery contained 50 square feet of coated surface; and he found that it was capable of receiving a charge so high, that the discharge melted 45 inches of iron wire of $\frac{1}{150}$ part of an inch in diameter, which answers to about $\frac{7}{8}$ of an inch in length for each square foot, which was the greatest length of wire ever melted. We have no account of this experiment having been afterwards repeated on a scale of any considerable magnitude till the year 1785; when I constructed a battery for the Teylerian society, at Haerlem, containing 135 square feet of coated surface. With this battery 180 inches of the same sort of wire was melted, which seemed to be much more than in proportion to the size of the battery, as this was about 1,3 inches for each square foot. This battery was afterwards increased to 225 square feet of coating, and with this 300 inches of the same sort of wire melted, which was also at the rate of 1,3 inches for each square foot. Some time after this, I made another battery for the same society, containing 550 square feet of coating, composed of 100 jars of $5\frac{1}{2}$ square feet each.

* Communicated by the author. † Philos. Trans.

each. The ſame ſort of wire was not tried with this; but it could be calculated from other ſorts of wire which it melted, that it was capable of melting 655 inches, being alſo at the rate of 1,3 inch for each ſquare foot. This increaſe of power, which is almoſt double that of Mr. Nairne, might be attributed to the acting power of the machine; for though Mr. Nairne's machine poſſeſſed the ſtrongeſt acting power of any machine made at that time, yet it could not be ſuppoſed to poſſeſs that high charging property of the Haerlem machine.

Since my return to London, I have made ſeveral batteries, commonly compoſed of 15 jars, each containing 168 ſquare inches of coated ſurface, conſequently the whole battery contains 17 ſquare feet of coating. This battery, according to the proportion of that made by Mr. Nairne, ſhould fuſe 6,3 inches, and in proportion to the Haerlem batteries, it ought to fuſe 22 inches; but inſtead of following that proportion, it is found to fuſe 60 inches, which is an aſtoniſhing increaſe of force. For the battery is only about one-third part of that of Mr. Nairne, and fuſes a much greater length of wire; and though it is only $\frac{1}{32}$ part of that at Haerlem, yet it fuſes $\frac{1}{10}$ of the length of wire. It ſeems difficult at firſt ſight to account for this advantage. I have before remarked, that the proportional difference between the charge of the battery at Haerlem and Mr. Nairne's, might be accounted for, from the high charging power of the great machine; but the reſult of the laſt-mentioned experiments overturns that notion; as it can by no means be ſuppoſed, that a ſingle 2-feet plate machine, which I have uſed to charge the battery of 17 ſquare feet ſo high as to fuſe 60 inches, can have a higher charging power than that at Haerlem; ſo that it muſt proceed from ſome other cauſe. It might be queſtioned whether all the batteries were alike judiciouſly conſtructed. As to Mr. Nairne's, it had certainly faults, both with reſpect to the coating and the mounting of the jars; but the batteries at Haerlem were as judiciouſly conſtructed as my preſent one, which I am ſpeaking of, and which exceeds them in ſuch an aſtoniſhing degree in its proportional force. The only difference between my preſent batteries and theſe at Haerlem is in the glaſs. They were compoſed of glaſs, blown in Bohemia, and thoſe which I make here are of white flint-glaſs. I mention this fact, but I am not inclined to think that the cauſe of the difference depends on the glaſs, becauſe I remember to have melted the ſame quantity of wire with one jar of that kind of glaſs when in Amſterdam, as I do at preſent with white flint-glaſs; ſo that it only remains now to be ſought for in the manner of uſing or charging each battery, and here we ſhall probably find a means of ſolving this paradox.

With regard to the batteries at Haerlem, they were never attempted to be charged but in dry weather, being ſuch as was then commonly called favourable for electrical experiments. There was no convenience in the room where the machine and batteries were uſed, for making a fire, which was therefore ill calculated for electrical experiments: the batteries previous to charging, were made as clean and dry as poſſible; and if they received a charge ſo high as to cauſe a ſpontaneous exploſion, they were then looked upon to be in their moſt favourable ſtate.

It was about this time that we were told by Mr. Brooke, that a coated jar would take a higher charge when dirty, than when clean; but the degree of dirtyneſs was ſo ill defined, that I muſt own I never could dirty a ſingle jar ſo as to anſwer, or to come near what was ſaid of it; and to pretend to bring all the jars in a large battery, containing upwards of two hundred, into that ſtate of dirtyneſs was never attempted; neither does it appear that Mr. Brooke ever thought

thought of dirtying his battery jars, as he only mentions trying two small bottles, whose charging property was very differently increased by his method of dirtying.

Some time afterwards, in the year 1792, I happened casually to discover that a coated jar, when it was a little dampish in the inside above the coating (which is always the case when a jar is fresh coated), would take a higher charge than it would do after it had been coated for some time, and was quite dry in the inside; and also, if the atmosphere was in a moist state, and the jar not dryed in the inside, it would take an equally high charge. From this it appeared evident to me, that if I could, by any means, render the inside of jars damp, it would answer the same purpose. Breathing into a jar was tryed, and the success was such, that it would receive and retain nearly double the quantity of electric fluid it could retain when dry: and in trying to fuse wire with the charge of one jar in a dry state, no more than 5 inches could be fused, though after breathing into it, 12 inches were fused.

This method appeared at first sight to have increased the force to more than double; but notwithstanding so evident and striking an effect, I did not think of trying what would be the result of charging a battery, after the jars had been breathed into; being deterred, as I suppose, from the idea of its being so contradictory to the common method of using batteries, which was never attempted to be done, but when the atmosphere was in a tolerably dry state, and the jars previously cleaned. But in March 1796, being engaged in a course of experiments, when the atmosphere was so very dry, that a spontaneous discharge always took place before I had a sufficient force to answer my purpose, it then occurred to me to try what the effect of breathing into the jars of the battery would be. In this trial, or research, it became necessary first, to ascertain the real charge that the battery was capable of receiving, before a voluntary explosion took place. This battery contained 17 square feet of coated glass, and was composed of 15 jars: it was found in the then state of the atmosphere to be incapable of fusing a greater length of wire than 18 inches. But after breathing into each jar through a glass tube, it took a charge which fused 60 inches, to my very great surprise and satisfaction, as I then thought I had obtained a method of making one battery perform the function of three; because three times the quantity of wire was fused, as appears by comparing this with what had been performed, by increasing the surface of batteries by former electricians. This notion seemed to be justified, by observing in Dr. van Marum's works, that I had enlarged his batteries at three different times; his first contained 135 square feet coating, the second 225 square feet, and the third 550; and the highest charge of the first was just sufficient to fuse 180 inches of iron wire of $\frac{1}{150}$ of an inch diameter, or 6 inches of iron wire of $\frac{1}{40}$ of an inch diameter; the highest charge of the second fused 300 inches of the first-mentioned wire, or 10 inches of the last-mentioned; the highest charge of the third fused 25 inches. We find that these batteries increased in power in the same proportion as the coated surface was increased. I was present when the wire was fused by the two first-mentioned batteries, but at the third not; however, we have no reason to doubt Dr. van Marum's report. These experiments supported me in my first notion, that I had discovered a new method of increasing the force of a battery to three times its usual power; but being unable to account for it to my own satisfaction, I resolved to make a course of experiments, in order to throw some light on the subject.

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The chief experiments which have been made on the force of batteries, by Mr. Brooke*, at Norwich, in the year 1786, and by Dr. Van Marum, in 1785 and 1795. The refults were very different. Some experiments which I made in Holland, and afterwards repeated here, did not feem to confirm either of the two. All that had been done either by Dr. van Marum, or myfelf, was done without the help of fuch an electrometer as could indicate the proportional quantities of electric fluid with a fufficient degree of accuracy.

Mr. Brooke was poffeffed of an inftrument of his own invention, with which it was poffible to afcertain the comparative ftrength, if managed with the fame dexterity as Mr. Brooke himfelf poffeffes. But this inftrument came fo high in price, and was fo very difficult in its ufe, that few electricians provided themfelves with it; which, perhaps, is one reafon, why this fubject has fo long remained in obfcurity. I have lately had the good fortune to invent an electrometer which has all the properties that fuch experiments require, and is very fimple and eafy in its ufe; and with this I found myfelf enabled to go through fuch experiments as were neceffary, with greater accuracy than any which had been made before.

The electrometer is reprefented in Pl. xxiii. G H is a long fquare piece of wood, about 18 inches long, and fix inches broad, in which are fixed three glafs fupports, D E F, mounted with brafs balls, *a b c*. Under the brafs ball *a*, is a long brafs hook; the ball *c* is made of two hemifpheres, the under one being fixed to the brafs mounting, and the upper turned with a groove to fhut upon it, fo that it can be taken off at pleafure. The ball *b* has a brafs tube fixed to it, about three inches long, cemented on to the top of F, and the fame ball has a hole at the top, of about one-half inch diameter, correfponding with the infide of the tube. A B is a ftraight brafs wire, with a knife-edged centre in the middle, placed a little below the centre of gravity, and equally balanced with a hollow brafs ball at each end, the centre, or axis, refting upon a proper fhaped piece of brafs fixed in the infide of the ball *c*; that fide of the hemifphere towards *c* is cut open, to permit the end *c* A of the balance to defcend till it touches the ball *a*, and the upper hemifphere C is alfo cut open to permit the end *c* B to afcend; *i* is a weight, weighing a certain number of grains, and made in the form of a pin with a broad head; the ball B has two holes, one at the top, and the other at the bottom; the upper hole is fo wide, as to let the head of the pin pafs through it, but to ftop at the under one, with its fhank hanging freely in *b*; a number of fuch pins are commonly made to each electrometer of different weights; *k* is a common Henley's quadrant electrometer, and when in ufe, it is fcrewed upon the top of *c*.

It is evident from the conftruction, that if the foot ftand horizontal, and the ball B be made to touch *b*, it will remain in that pofition without the help of the weight *i*; and if it

* Though I had read Mr. Brooke's book, as I thought, with a fufficient degree of attention when it was firft publifhed, I did not, till lately, obferve that it contained any experiments relating to this fubject, till I begun to write this paper, and had occafion to look into his book for fome references. I believe thefe experiments had efcaped Dr. van Marum's notice likewife, as I never heard him fpeak of them when he was making others of the fame kind. Though Mr. Brooke's experiments were conducted with much fkill and intelligence, they are fo confufedly arranged, that this had entirely efcaped my notice; and I doubt not but that it had alfo efcaped the notice of feveral other electricians.

fhould

should by any means receive a very low charge of electric fluid, the two balls *b*, B, will repel each other; B will begin to ascend, and, on account of the centre of gravity being above the centre of motion, the ascension will continue till A rest upon *a*. If the balance be set again horizontal, and a pin *i*, of any small weight, be put into its place in B, it will cause B to rest upon *b*, with a pressure equal to that weight, so that more electric fluid must be communicated than before, before the balls will separate; and as the weight in B is increased or diminished, a greater or less quantity of electric fluid will be required to effect a separation.

When this instrument is to be applied to a jar, or battery, for which purpose it was invented, one end of a wire, L, must be inserted into a hole in *b*, and the other end into a hole of any ball proceeding from the inside of a battery, as M*: *k* must be screwed upon c, with its index towards A; the reason of this instrument being added, is to shew, by the index continuing to rise, that the charge of the battery is increasing, because the other part of the instrument does not act till the battery has received its required charge.

If this instrument be examined with attention, it will be found to consist of three electrometers; and answers three different purposes, namely, a Henly's electrometer, Lane's discharging electrometer, and Brooke's steelyard electrometer; the first not improved, but the two last, which were very defective when first invented, I flatter myself are here brought to perfection. As the only use of Henley's electrometer to this instrument is, as I have said before, to shew, by its continuing to increase in divergency, that the battery continues to receive a still stronger charge, it required no improvement; but Lane's electrometer, in its primitive state, could by no means answer the required purpose for batteries, because the ball intended to discharge the battery, was necessarily placed so near to the ball of the battery, that dust and fibrous particles were always attracted by and adhered between the two balls, so as to retard the charging, and often render a high charge impossible: whereas, in this, they are placed at four inches asunder; and when the desired height of charge is obtained, and not before, the ball of the electrometer moves of itself nearer to the ball which is connected with the outside of the battery, and causes a discharge. The defects in Brooke's steelyard electrometer were, 1st, that it could not cause a discharge, and 2dly, the difficulty of observing the first separation of the balls caused great error. If it were not placed in an advantageous light (which the nature of the experiments could not always permit), it would not be seen, without the attention of an assistant, which is sometimes unpleasant, and cannot always be commanded. But the instrument which I have described, requires no attention or assistance; for as soon as the separation takes place between B and *b*, the ball A descends, and discharges the battery of itself.

By this combination and improvements, we possess in the present instrument all that can ever be required of an electrometer; namely, by *k*, we see the progress of the charge; by the separation of B, *b*, we have the repulsive power in weight; and by the ball A, the discharge is caused, when the charge has acquired the strength proposed.

* A chain, or wire, or any body through which the charge is to pass, must be hung to the hook at *m*, and carried from thence to the outside of the battery, as is represented by the line N.

Experiments

Experiments made with a View to determine in what Degree the charging Capacity of coated Jars is increaſed by breathing into them before the charging.

Experiment 1. Prepare the electrometer in the manner ſhewn in the plate, with the jar M annexed, which contains about 168 ſquare inches of coating *; put into B the pin, marked 15; take two inches of watch-pendulum wire, fix to each end a pair of ſpring tongs, as is repreſented at G *m*, hook one end to *m*, and the other to the wire N, communicating with the outſide of the jar; let the uncoated part of the jar be made very clean and dry; and let the prime conductor of an electrical machine, or a wire proceeding from it, touch the wire L; then if the machine be put in motion, the jar and electrometer will charge, as will be ſeen by the riſing of the index of *k*, and when charged high enough, B will be repelled by *b*, and A will deſcend and diſcharge the jar through the wire, which was confined in the tongs, and the wire will be fuſed and run into balls.

Experiment 2. Put into the tongs eight inches of the ſame ſort of wire as before, hang one pair of tongs to the hook *m*, and apply the other to the wire which forms the outſide communication: take out the pin in B, and put in its ſtead one marked 30; all the other part of the apparatus remaining as before, and the uncoated part of the jar being previouſly cleaned and dried; the machine being then put in motion, the jar and electrometer will charge, as is ſhewn by the riſing of the index as before; but as ſoon as the jar has received a greater quantity of electric fluid than before, a ſpontaneous exploſion will happen without affecting the balls B *b*, becauſe the diſcharge will have paſſed along the uncoated part of the jar from the inſide coating to the outſide: whence it follows, that while the jar remains in that clean ſtate, it is incapable of receiving a charge high enough to affect the balls, or even a higher charge than it had received in the firſt experiment. Let the uncoated part of the jar be therefore rendered, in a ſlight degree, damp; which is eaſily done, by breathing into the inſide, through a glaſs tube; put the machine in motion, and no ſpontaneous exploſion will happen, but the balls B *b* will repel, as in the firſt experiment, and the diſcharge will happen from A to *a*, and paſs through the wire placed in the circuit; and though it was eight inches, it will be fuſed in the ſame degree as two inches in the laſt experiment, namely, the wire ſeen red hot the whole length, and then fall into balls.

Very different degrees of fuſion are cauſed by electric diſcharges, which may cauſe great miſtakes, if not well attended to. It is proper to adhere to the degree above-mentioned, and particular care ought to be taken to lay the wire, intended for fuſion, ſtraight, without any bendings or angles in it. The wire uſed in the two laſt experiments, was that which is commonly called watch-pendulum wire, which is flatted; and as it approaches very near to ſuch a ſharp edge as might be ſuppoſed to affect the experiment, by permitting a diſſipation of the electric fluid in its paſſage, round wires were tried, and the reſult was the ſame.

* Take out the pin in B, and obſerve whether the ball B will remain at reſt upon *b*; if not, turn the adjuſting ſcrew at C, till it juſt remains upon A.

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By the last experiment it appears, that breathing into the jar had increased its charging capacity nearly in the same proportion as it had done the batteries: after breathing, it received a charge sufficient to fuse four times the length of wire it did when clean; but by the weight in the electrometer, and also by the greater number of revolutions given before the discharge happened, it might be supposed that the jar had received only a double charge.

The following experiments are intended to shew the lengths of wire, which are just fused by various quantities of electric fluid at the same intensity.

Experiment 3. For this purpose, a second jar was placed at the wire L, the pin marked 30 was taken out, and 15 put in its place, two inches of the same sort of wire as used in the last experiment was placed in the circuit, every other part of the apparatus remaining unaltered; the machine was then put in motion, till B begun to ascend, when it was stopped, and before A could reach *a*, one of the jars was pushed from the wire L (to do which, there is always sufficient time while the electrometer is in motion), the discharge was effected, and the two inches of wire was just fused.

Experiment 4. The jar which was pushed away in the last experiment was discharged, and placed at the wire L, as before, and eight inches of the same sort of wire placed in the circuit; the outside coating of the jars either touched each other, or had a metallic communication. All the other part of the apparatus remained as before, and the machine was put in motion, till B begun to ascend; the jar was not removed, as in the last experiment, but suffered to discharge with the other, and the eight inches of wire was fused in the same degree as the two inches in the last experiments.

It is evident from the position of the apparatus, that the quantity of electric fluid discharged in the last experiment must be double that of the former; yet, in repeating the experiment, I had different results, which made me again suspect the edges of the wire, I therefore resolved to take round wire, and of as large a diameter as could be conveniently fused.

Experiment 5, *with three jars.* Iron wire of $\frac{1}{150}$ part of an inch in diameter, and six inches in length, was placed in the circuit; three jars were placed so that the balls proceeding from their insides touched the wire L, and their outside coatings touched each other. The machine was turned till B begun to ascend, the discharge was caused, and the whole length of the wire was just run into balls.

Experiment 6, *with three jars, one removed.* Two inches of the same sort of wire was placed in the circuit in the same manner as the last, and the three jars remained; the machine was turned till B begun to ascend, then one of the jars was drawn away, consequently only two discharged, and the wire just run into balls as the last.

Experiment 7, *with four jars.* Wire of $\frac{1}{150}$ part of an inch was taken, and four jars placed in contact with the wire L, with their outside coatings in contact with each other, and eight inches of wire was placed in the circuit; the weight in the electrometer remained as before; the machine was then put in motion till B begun to ascend, the discharge was effected, and the wire was fused, and run into balls. The experiment was repeated with the same sort of wire $8\frac{1}{2}$ inches long; the discharge was just sufficient to run it into balls.

Repeated with nine inches of the same sort of wire, and the discharge caused it to be red hot the whole length.

Experiment

Experiment 8, *with four jars, two removed.* Two inches of the ſame ſort of wire was placed in the circuit, all the jars remaining as in the laſt experiment, the machine put in motion, till B begun to aſcend, then two of the jars were drawn away; the diſcharge was cauſed, and the wire was fuſed, and run into balls.

Repeated with the ſame ſort of wire $2\frac{1}{2}$ inches long, the diſcharge cauſed it to be red hot the whole length.

Experiment 9, *with fourteen jars.* Wire of $\frac{1}{100}$ part of an inch diameter was taken, eight inches long, and proceeded according to Experiment 7; it was fuſed and run into balls.

Experiment 10, *with fourteen jars, ſeven removed.* Two inches of the ſame ſort of wire was taken, and proceeded with, according to Experiment 8; it was fuſed and run into balls.

The reſult of the foregoing experiments proves ſufficiently, that double quantities of electric fluid, in the form of a diſcharge, will melt four times the length of wire of a certain diameter; and Experiments 5 and 6 prove that when one-third part is added to two, three times the length of wire was fuſed.

Theſe experiments give reaſon to apprehend ſome error in Dr. van Marum's experiments, becauſe he found his batteries to increaſe in power only in the ſame proportion as the coated ſurface was increaſed, viz. that double ſurface of coated glaſs only could fuſe double lengths of wire of the ſame diameter.

The doctor might, perhaps, have been led into a miſtake in the following manner: firſt, he may not have charged the batteries to an equal height, as he did not, at that time, poſſeſs an electrometer of ſufficient accuracy for that purpoſe; and, ſecondly, he may not have been aware of the different degrees of fuſion cauſed by electric diſcharges, but only judged of the force by the wires being converted into balls; by which great miſtakes may happen. For if a wire be taken 18 inches long, and of ſuch a diameter, that when a jar or battery is charged to ſuch a height as juſt to cauſe it to run into balls, much ſhorter lengths of that ſame ſort of wire may be ſubjected to the ſame force, and ſtill be only converted into balls by it; even if only ſeven inches were taken, nothing but balls will appear; the only difference will be, that the balls will be ſmaller, and diſperſed to a greater diſtance, which might be eaſily overlooked. If ſix inches of the ſame ſort of wire be taken, it will be converted into balls and flocculi, or brown oxyde of iron; ſo that to be accurate in this point, the loweſt degree of fuſion muſt be had, which is known when the charge has paſſed, by the wire being ſeen red-hot the whole length, and afterwards run into balls.

Having now ſufficiently proved by experiment, in what proportion different quantities of electric fluid act upon different lengths of wire, which was required to be known, in order to explain in what proportion the charging capacity of a jar or battery is increaſed by breathing into it, before the charging begins, I ſhall proceed in the next place to explain this point.

The opinion that I had at firſt entertained (though ſupported by Dr. van Marum's experiments), that I had found out a method of increaſing the charging capacity of batteries to three times their uſual force, was not ſupported by the facts that the uſual power of a clean and dry battery, containing 17 ſquare feet coated ſurface, namely, that of fuſing from 18 to 22 inches of iron wire of $\frac{1}{150}$ part of an inch in diameter, will be increaſed by breathing into the jar, ſo as to become capable of fuſing 60 inches. If the firſt-mentioned effect be taken at a mean

a mean, it will be 20, then the increased effect, gained by breathing, will be just $\frac{2}{5}$, as determined by the wire; and experiments 5 and 6 prove, that in order to produce such an increased effect, an addition of $\frac{2}{7}$ part of the coated surface must be added to the battery, which is about 816 square inches. This would amount to an addition of 54 square inches to each jar; or, in other words, if that quantity of coating could be added to each jar, the same effect would be produced as when breathed into. But this would require the coating to be within an inch of the top, which would render the battery unchargeable, at least, to that degree. A battery of 15 jars constructed in the usual manner, will, therefore, by this treatment, become equivalant in power to 21 jars of the same kind, if clean and dry.

To explain the effect of breathing into the jars, appears to be a matter of some difficulty. This experiment has been shewn to several electricians, and different opinions have been advanced, most of which seem to imply, that breathing acts as a coating to the uncoated part, which will appear in the sequel to be absurd. Mr. Nicholson's opinion (see Philos. Journal, II. 219) comes much nearer to the truth, though it does not appear to me to be sufficient to account for the effect produced. I admit, with him, that a spontaneous explosion over the uncoated part is most commonly caused by undulation; but that this undulation is caused by the discharging of different charged zones, will be difficult to prove, because such zones cannot exist upon clean and dry glass.

When the uncoated part of a Leyden jar is made perfectly clean and dry, and the jar set to the conductor of a machine in action, it will begin to charge, and, while charging, the coated part of the jar, and the wire which is connected with it, become equally charged, and each endeavours to throw off that surplus of electric fluid which is forcing into them; the coating from its edges upwards, and that part of the wire which is above the coating and within the jar, will endeavour to throw it in all directions, which will cause it to be surrounded by an electric atmosphere, increasing in density as the charge increases. This atmosphere, together with that given out by the coating, fills the whole jar. Part of the electric fluid forced into the coating enters the surface of the glass, but the uncoated part, being clean and dry, both withinside and without, the inside resists the fluids entering its surface, which is kept suspended at a distance, because the natural electric fluid contained on the outside, finds no means of escape. But the action of the machine still continuing, presses it still closer to the surface, and at last overcomes that resisting force, and some of the particles on the outside give way, which causes an undulation in the inside, and the electric fluid closes instantly in upon its inside surface, and forces a greater quantity from the outside. Flashes, or coruscations, are thus caused, which are always seen when a jar is charging in the abovementioned circumstances: the charge still continuing to be made, forces another quantity from another part of the outside of the jar, and causes a second coruscation and undulation, which may be so strong as to cause a spontaneous discharge; or two or three more coruscations and undulations may happen, before the discharge, according to the steadiness or unsteadiness of the action of the machine, the quantity of electric fluid thrown off from the outside at each undulation, and also the degree of dryness and cleanness of the uncoated part of the jar. A discharge sometimes happens without having previously occasioned any perceptible coruscation. This is the case when the first undulation has been so strong, as to cause the whole discharge

discharge with the first coruscation, the one being so quickly followed by the other that it is imperceptible.

A jar will sometimes, while it is charging, give a great many small coruscations, quickly succeeding each other, which afterwards cease without having caused a spontaneous explosion, though the action of the machine be continued. This happens when the uncoated part is nearly clean and dry, but not perfectly so; its surface still containing some conducting particles, but not so connected, that the electric fluid can pass from one to the other without leaps, or small corruscations on the outside, which permit the electric fluid to spread gradually over its inside surface, and prevent the undulations from being so strong as to cause a discharge.

After this explanation of the cause of the flashes, or coruscations, which are seen upon the uncoated part of a jar while charging, and also that such coruscations produce undulations, which terminate in a spontaneous explosion; it remains now to explain how a jar is charged when the coruscations are prevented by breathing upon the uncoated part.

When a coated jar is breathed into, and then subjected to the process of charging, the electric fluid is forced into it, along the wire in the inside to the coating, where it instantly and equally spreads itself over the whole coated part, and at the same time, though with difficulty, and consequently gradually, it spreads itself over the uncoated part, taking the condensed film of humidity for its conductor, as it proceeds from the edges of the coating upwards towards the mouth of the jar, according to the arrangement of the particles of moisture, and rises higher or lower, depending entirely on their arrangement, and the force with which it is repelled from the machine. If the conducting particles be almost uniformly diffused over the uncoated part, the whole jar, in the inside, will become charged, though the uncoated part will be charged in a much less degree than the coated, on account of the imperfection of the conducting particles which has adhered to its surface; no coruscations will be perceived, on account of the gradual and equal diffusion of the electric fluid over its inside surface: and though the charging be continued, yet, if the exhaled conducting particles be favourably diffused, no spontaneous explosion will happen from one coating to the other, along the uncoated surface, but the jar will either be perforated, or, if it be of sufficient strength to resist that effect, the electric fluid will be seen to run in a stream over the mouth of the jar, as quickly as the machine supplies it. Whenever a spontaneous electric explosion happens, it must be from a body of sufficient bulk and conducting property to contain that quantity of electric fluid at that point from which it explodes, otherwise no explosion ever happens. But the humid conducting particles are just sufficient merely to admit the electric fluid, by the action of the machine, to be spread over the surface of the glass, but in no part of sufficient density, either to receive, or contain an explosion. If, therefore, a spontaneous explosion do happen, it must either proceed from the inside coating, or the wire which is connected with it to the outside; and, if we examine the state of the coating, we shall understand, that the edge of the coating (from which part only it is ever possible to explode), and also above it, to a short distance upwards, is as strongly charged as the coated part; and by the action of the machine it is so strongly loaded with electric fluid, that it is repulsive in all directions, which keeps back, or entirely stops, a spontaneous explosion from the

the edge of the coating. With regard to the wire, the only place from which it explodes spontaneously, is that part which is nearly of an equal height with the edge of the mouth of the jar. The fluid is nearly as much condensed on this part as on the other, so that an explosion from the wire is hindered by the same cause as from the coating. A jar under such circumstances cannot, therefore, explode spontaneously; but the fluid will run over the edge of the jar as quickly as the machine furnishes it, when its charging capacity is full.

I have stated, at page 530, that a jar of the dimensions there given, being clean and dry, can only contain a charge sufficient to fuse 2 inches of a certain wire, and when breathed into, its charging capacity will be so much encreased, that it will contain a charge sufficient to fuse 8 inches of the same sort of wire; and a battery of 15 jars, in the first-mentioned state, can only fuse 20 inches, and in the last-mentioned, 60 inches. This increased charging capacity, proceeds, no doubt, from the particles of moisture, though not from their acting as a coating, as has been supposed, but by their being brought into a state or capacity of resisting a spontaneous explosion, so that a stronger charge is forced in upon the coated part. Some of the electric fluid which was forced upon the uncoated part to a certain height (perhaps ½ an inch, more or less, according to the degree of dampness, and the situation of the particles) may, indeed, be discharged along with that from the coated part; but this is of little importance, and by no means capable of producing that increased effect, which, as I have shewn by experiment, would require an addition of seven jars to a battery of fifteen.

II.

Discovery of Sulphate of Strontian, near Sodbury, in Gloucestershire. By G. S. GIBBES, B. M. F. R. S.

To Mr. NICHOLSON.

SIR,

IF the following imperfect analysis should meet your approbation, I shall feel myself flattered by seeing it noticed in your excellent monthly publication.

I have the honour to be,

Sir,

Your humble servant,

GEORGE SMITH GIBBES.

Bath,
No. 28, Gay-street.

A friend of mine, the Rev. Mr. Richardson, some little time since, shewed me a specimen of a substance which, he said, was found in great abundance in the neighbourhood of Sodbury, in Gloucestershire, where it was used for the purpose of making gravel walks. The stone was composed of a vast number of small crystals, which cohered together with but little force. The crystals were easily reduced to powder, and were not affected by any acid. I exposed equal parts of these crystals in the state of a fine powder and charcoal to the action

action of heat, and I found, on pouring the muriatic acid on the ſubſtance, when cool, that there was a great diſengagement of ſulphurated hydrogenous gas.

I filtered this ſolution in muriatic acid, and I found the cryſtals exactly ſimilar to thoſe which I had often formed, by adding the muriatic acid to the ſtrontian earth, found in Scotland. This muriate poſſeſſed the property of changing the flame of the candle to a moſt beautiful red colour. A copious precipitation was formed, when the ſulphuric acid was added to a ſolution of this muriate. From theſe experiments, and from the external characters of the ſtone, I have no heſitation in believing, that it is compoſed of the ſulphuric acid and ſtrontian earth.

P. S. Should you wiſh to poſſeſs a ſpecimen of this ſubſtance, on receiving a line from you, I will take the firſt opportunity of ſending one to you.

III.

On the Corundum Stone, from Aſia. By the Right Honourable CHARLES GREVILLE, *F. R. S.*

(Continued from p. 485, Vol. II.)

MR. Tranckell, who reſides in Ceylon, and from whoſe communications I derived lately much information, had, about five years ago, a ſapphire, the greater part blue, and the remainder of a ruby colour. I ſaw, in Romé de L'Iſle's collection, at Paris, a ſmall gem, which was yellow, blue, and red, in diſtinct ſpots, and he called it oriental ruby. M. de la Metherie, to avoid the confuſion of the denomination oriental ruby, with octoedral ruby, calls it a ſapphire: with more correctneſs, I think, the above-mentionened gems ſhould be claſſed as argillaceous, under the denomination of corundum. I am not uninformed, that corundum is ſaid to be found in France. The Count de Bournon is convinced, that the ſpecimens mentioned in Crell's Journal, as having been found by him in a granite in the Forez, were corundum. M. Morveau alſo ſays, he found it in Bretagne; but the Abbé Hauy, in No. 28, of the Journal des Mines, aſſerts, that the corundum, found in France, is titanite; he does not ſay whether this obſervation extends both to the corundum of Bretagne and that of the Forez. In the ſame manner I had obſerved in the ſpecimens, which Mr. Raſpe called jade, or a new ſubſtance from Tiree, on the weſt coaſt of Scotland, a great reſemblance to corundum; but, having then only had a curſory view of the ſubſtance, I am indebted to Mr. Hatchett for the examination of a ſpecimen of it, which he had from Mr. Raſpe's collection. The Tiree ſtone reſembles cryſtallized corundum of the coaſt, in texture and colour; it is alſo as refractory, when examined by the blow-pipe, with different fluxes. Its ſpecific gravity is 3,049; conſequently nearer the ſpecific gravity of pure corundum than the above-mentioned lump, 2,785, and the matrix of corundum, 2,768. The Tiree ſtone will ſcratch glaſs readily, but not rock cryſtal; its hardneſs, therefore, correſponds with that of the matrix of corundum. The ſubſtance of the lump, deſcribed in page 481, cuts glaſs and rock cryſtal, and the Tiree ſtone, readily.

It will, therefore, be ſufficient for me to ſay, that there is great probability corundum may be

be found in Great Britain, and on the Continent of Europe, as well as in Asia; and the above slight essays may show, that observations on corundum, in its different states of purity, may lead to accurate distinction between substances hitherto imperfectly known, and will lead to a revision of the siliceous genus, whereby the argillaceous gems may obtain its due pre-eminence in mineralogy. When gems, by art, or by rolling in the beds of rivers, have been deprived of the angles of their crystals, they are unavoidably subjected to uncertain external characters, which even great practice cannot render certain; and hence the unwillingness of European Jewellers to deal in coloured gems. I have some specimens of a sapphire-blue stone, India cut, very small and pellucid; they were purchased in India as sapphires, and were supposed to be fluor by a lapidary in London, but are cyanite. The above could scarcely have happened, if the stones had been of sufficient size and value to require much examination, the weight and degree of hardness being exceedingly deficient. The colour, therefore, will not be a safe guide. The diamond, whether white, blue, red, yellow, or green, can be distinguished by its crystal, or by its specific gravity and hardness, or, when it is polished, by its lustre. Other stones, which compose the order of gems, might equally depend on their crystallization, specific gravity, polish, and hardness, for a distinct arrangement. The near relation of argil, which Bergman gave to this order, is daily confirmed; and it will be, perhaps, to Mr. Klaproth, more than to any other existing chemist, that we shall owe our correct information on the subject of other gems, as we do on the subject of corundum.

Many of the varieties of corundum, particularly the coloured and transparent sorts, with their regular crystallizations, are yet *desiderata*. Many crystallized stones, from defect of colour, lustre, &c. are of little value in the market, such as jargon, chrysolite, tourmaline; and an infinity of unnamed stones of Ceylon, Pegu, Siam, &c. would be valuable to the mineralogist, if obtained adhering to their strata, and in crystals, when external form is not obliterated. I have no doubt, when it is known how much such information will tend to illustrate the history of the earth, and particularly that of gems: the spirit of enquiry, so laudably afloat in British India, will be directed to attain it. I have not heard of any metallic veins being found in corundum, unless a stone, which Alonso Barba, *lib.* 1, *c.* 13, describes, should give an instance. "The chumpi, so called from its grey colour, is a stone of the nature of emery, and contains iron; it is of a dull lustre, difficult to work, because it resists fire long. It is found at Potosi, at Chocaya, and other places, with the minerals, negrillos, and rosicleres."

Having mentioned the varieties of crystallized and amorphous corundum, and the miscellaneous facts relative to my collection of that substance, from India and China, it might be sufficient to give an *icon* of the crystal, and close a paper already prolix; but having with satisfaction observed, within the last years, the science of mineralogy gaining ground in Great Britain, from the knowledge acquired by several gentlemen, who have examined the mines, and formed personal acquaintances with the most learned and experienced men on the continent, and also from ingenious foreigners, who have communicated their observations on English fossils, and connected the n with the most approved systems, it may, perhaps, be accepted as a sufficient apology for what follows; that I consider it as a desideratum to English mineralogists, to be invited to a preference of permanent characters, which the study of crystallization has collected, and which promises

promises to be a certain method of ascertaining the laws, by which elective attraction arranges and combines molecules of matter. It is true, the progress of crystallography has been extremely slow, and different nations have contributed to its present improvement. It is rather remarkable, that the earliest treatise on metallurgy, of authority, was published in Italy, by Vanoccius Biringuccius, just before Agricola published his Treatise, in 1546, in Germany; and the first treatise, on the Structure of Crystals, I know, is also from Italy, by Nicolas Steno; *Prodromus Dissertationis de Solido intra Solidum naturaliter contento. Florientiæ*, 1669, *in 4to.* A work of great merit. Louis Bourguet of Neufchatel, in his *Lettres sur la Formation des Sels et des Crystaux. Amst.* 1729, 12*mo.* connected, by observation and measure, triangular and rhomboidal, and cubic, and pyramidal tetraedal molecules, for all different substances. His contemporary, Maurice Antoine Capeller*, attempted to deduce a system from geometrical principles; and in this state did Linnæus find the subject, when he attempted to reduce the science of minerals to external characters, and crystallized bodies to salts.

None of the observations of Linnæus will prove useless to science; but his system alarmed the chemists and mineralogists, who rejected every other criterion, than internal character from analysis, and the system of Cronstedt was preferred by general assent. By this means, a spirit of controversy deprived the chemist and lythologist of mutual assistance; and the general opinion was correct, on the supposition, that a mixed system of chemical and external characters would be irreconcileable; but it has been admitted, even by those who most decidedly opposed Linnæus's system, that the best system of mineralogy should be founded on external and internal characters combined†. Among the few, who ventured to express their obligations, at the same time, to Linnæus, and to Cronstedt, was Baron Born, whose abilities and character, in addition to his distinction, as one of the counsellors of mines of his Imperial Majesty, obtained his inrolment among the fellows of the Royal Society. He connected the intrinsic and extrinsic characters of minerals, in the Index Fossilium, which he published in 1772. In Sweden, Bergman's Treatise on the Forms of Crystals, published in the Upsal Transactions, in 1773, was a more authoritative recommendation to the investigation of the principles of crystallization; and it can be of little importance for me to add, that since I have possessed the collection of Baron Born, in 1773, I have had every confirmation of the same opinion. The progress of chemistry, and of crystallography, applied to mineralogy, has rendered the examination of strata, and of mines, a source of amusement as well as instruction; and the arrangement of interesting facts, in the chemistry and mechanism of nature, suits my occasional researches in geology, which, from variety of avocations and circumstances, have been very much interrupted. My acknowledgment of obligation to the learned, who have made this progress in science, is the best recommendation I can give to others to examine their works. Those whose talents and time are devoted to the investigation of every mineral substance can have no respite to their labour; minerals, in every state of their

* Prodomus Crystallographiæ, &c. and Litteræ ad Scheuzerum, de Crystallorum Generatione. Act. Nat. Cur. vol. 14. Append. p. 9.

† Nullum itaque est dubium, quin hujusmodi methodus mixta, quæ notis characteristicis tam extrinsecis quam intrinsecis simul combinatis, est superstructa, proxime ad naturalem accedens, maximam indicans symmetriam, reliques sit præferenda methodis. J. G. Wallerius, de Systemate Mineralogico rite condendo. §. 102.

formation,

formation, perfection, and decomposition, as they occur in mines, must have their qualities immediately ascertained, and be reserved for profit, or thrown away on the heap. The practical miner could not, without external characters, make any progress. The valuable minerals are soon pointed out by assay, and their appearance numbered. The accuracy of selection depended, in all periods, much on the experience of the miners. It remained for Mr. Werner to give the utmost degree of accuracy which irregular external characters can acquire, by fixing appropriate terms to all the characters which occur, and which the senses can discriminate. In 1774, he opened his system of external characters of minerals; and the perfection he has since given to it, has rendered it very general. The Leskean collection, arranged after Mr. Werner's method, has procured in Mr. Kirwan a powerful support to the introduction of that system in this country; and we have already some other valuable publications, to recommend and introduce other favourite systems of the continent. It is, therefore, at this time, the English mineralogist should be invited to examine, if not to prefer, permanent characters, so far as the progress of crystallography has collected them, or at least to give them a distinguished rank among external characters of bodies.

If prejudice too long has retarded the union of intrinsic and extrinsic characters, it has also occasioned a schism among the advocates of crystallography.

Romé de L'Isle, in the year 1772, published the first edition of his Essay on Crystallography, which he states to be a supplement to *Linnæus*; and, by the assistance of a very few friends, he was enabled to increase the number of crystals in a degree to assume the appearance of a system. He told me that the accuracy of his measurement of angles of minute crystals was the acquirement of great practice; but that the Count De Bournon, after a short practice, attained equal correctness, and afforded him assistance, which he acknowledges, in his second edition, to have received, particularly by the discovery of crystals in Dauphiné, Auvergne, Franche-Comté, &c.

The Abbé Hauy, an accurate and patient observer, and a good mathematician, considered crystallography as founded on certain laws, reducible to demonstration by calculation. In the beginning, the differences of Bourguet and Capeller were not more pointed than those of Romé de L'Isle, and the Abbé Hauy; but the progress of observation and calculation having demonstrated their mutual utility, the observer and measurer of crystals will now rest satisfied only when calculation confirms actual measurement. To the Abbé Hauy is also due a late scheme to simplify calculation, by expressing, according to algebraical formulæ, the different laws which determine the modification of crystals. So far as they are the result of calculation and measurement, we may admit the laws of calculation; for whenever the superposition, or subtraction, of simple or compound molecules, on a nucleus, shall, by calculation, give a series of planes and angles, which corresponds exactly to the angles and planes measured on natural crystals, it will amount to no more nor less than a demonstration of the rule or arrangement of elective attraction by figures.

These laws may be reduced to simple practice; for instance, the Abbé Hauy, by measuring the rhombic plane of corundum, found its two diagonals to be as two to three: which gives to

to its acute angle 81° 47′ 10″, and to its obtuſe angle 98° 12′ 50″; the ſame as martial vitriol*. The forms of fragments in corundum are all acute rhomboids. The coſine of the little angle in corundum is one-ſeventh of the radius; but in calcareous ſpar, the coſine is one-fifth of the radius; in ſchorl two-fifths of the radius; in the garnet, one-third; and in rock-cryſtals, one-ſeventeenth. Thus the application of general laws, to aſcertain conſtant character, after they ſhall have been fully verified, may be very ſimple and general. It will not require perfect cryſtals; for when cryſtals ſeparate into laminæ, which ſubdivide into fragments, and ſhew the form or arrangement of their molecules, it is eaſy, from ſuch fragments, to connect them with their primitive cryſtal, and, conſequently, with their claſs. It will be a great ſtep, to obtain one regular and permanent external character.—Attention to other characters will be neceſſary, to aſcertain the nature of the ſubſtance; and other external characters, ſuch as irregular fracture, colour, &c. muſt be reſorted to, when no permanent characters exiſt; but from their nature they are fallible, and, in fact, are ſeldom concluſive.

The progreſs of cryſtallography appearing, to me, of conſequence to the progreſs of mineralogy, induced me to deſire the Count de Bournon, above mentioned, one of the honourable victims to his allegiance to his king, to deſcribe ſuch cryſtals, in my collection, as ſhewed the different known modifications of corundum; which will develope the theory of cryſtallization, ſo far as is conſiſtent with the avowed object of this paper. The ſubject, I believe, has not hitherto been ſubmitted to the conſideration of this ſociety. The tranſlation of the Count de Bournon's deſcription has been carefully made, to preſerve its clearneſs; and, I hope, it will be favourably received by the Society, and make ſome amends for my tedious introduction. After it, I have added a table, connecting in one view the ſpecific gravities of corundum, &c. herein mentioned, with thoſe given by other authors.

An Analytical Deſcription of the Cryſtalline Forms of Corundum, from the Eaſt-Indies, and from China. By the Count DE BOURNON.

THE moſt uſual form of corundum is a regular hixædral priſm (Plate XXIV. Fig. 1.); in general, the ſurface of the cryſtal is rough, with little luſtre, owing to unfavourable circumſtances, under which it is cryſtallized.

The cryſtals of corundum, hitherto found, were not formed in cavaties, where each cryſtal being inſulated, its ſurface could preſerve that ſmoothneſs, and natural brilliancy, which are common to all ſubſtances that freely aſſume a cryſtalline form. Like the cryſtals of feldſpar which we meet with in the porphyroid granites, the corundum cryſtals have been enveloped, at the time of their cryſtallization, by the ſubſtance of the rock, which was forming at the ſame time with themſelves, in an imperfect and confuſed cryſtalline maſs; and the corundum cryſtal, before it had acquired its perfect ſolidity, neceſſarily received on its ſurface the im-

* This reſult is extracted from the Journal de Phyſique; but it appears, from the Journal des Mines, No. 28, that the Abbé Hauy has ſince rectified this meaſure, and given 86° 26′ for the acute angle, and 93° 34′ for the obtuſe angle.—G.

preſſion

preſſion of the different particles of the rock which enveloped them: this naturally renders the ſurface rough and dull. Cryſtals of feld-ſpar, formed in the granitic porphyroid rocks, exhibit the ſame kind of appearance, from the ſame cauſe.

The corundum cryſtals are, in general, opaque, or at leaſt they have only an imperfect tranſparency at the edges: when broken into thin fragments, the pieces are ſemi-tranſparent; when held between the eye and the light, and examined with a powerful lens, it will be perceived that their interior texture is rendered dull, by an infinite number of ſmall flaws, croſſing each other, much reſembling the medullary part of wood, when it is viewed in the ſame manner. The degree of tranſparency of the ſmall interſtices, which are between theſe flaws, is further evidence that this texture of ſmall flaws occaſions opacity, which augments in proportion to the thickneſs of the fragments.

This kind of internal ſtructure has alſo a very ſtrong analogy with that of feld-ſpar in granite and porphyry. The endeavour to ſplit theſe cryſtals in a direction, either perpendicular, or parallel to their axes, meets with a very conſiderable reſiſtance: they may, indeed, be broken in theſe directions; but the rugged and irregular ſurface of the broken parts, clearly proves that the direction in which the cryſtalline laminæ have been depoſited one upon another has not been followed.

The regular hexaedral priſm of theſe cryſtals cannot therefore be conſidered as the form of the nucleus of the cryſtal; and, conſequently, is not the primitive form of the cryſtals of this ſubſtance.

If, in order to diſcover the direction of the cryſtalline laminæ, a variety of cryſtals be examined, ſome will hardly fail to be met with, which, on their ſolid angles, formed by the junction of the ſides of the priſm, with the planes of the extremities, preſent ſmall iſoſceles triangles. Theſe are ſometimes greater, and ſometimes ſmaller, and form ſolid angles, of 122° 34′, with the extreme planes of the cryſtal. They are, in ſome inſtances, real faces of the cryſtal, but moſt frequently they evidently are the effect of ſome violence on that part. The ſmoothneſs and brilliancy of theſe ſmall faces, in the latter caſe, ſhew that a piece has been detached in the natural direction of cryſtalline laminæ. It is, indeed, much leſs difficult to ſeparate a portion of the cryſtal at thoſe angles, than at any other part; and in following the natural direction of the faces, with a little patience and dexterity, all the cryſtalline laminæ may be detached, and progreſſively increaſe the ſize of the triangular face.

This operation, however, cannot be done indiſcriminately on all the ſolid angles of the cryſtals, but only on the alternate ones at the ſame extremity, and in a contrary direction to each other. As to the other angles, they may be broken, but it is impoſſible to detach them. When, inſtead of the ſolid angles of an hexaedral priſm, ſmall triangular planes are met with (which frequently happens, whether cauſed by violence or otherwiſe), they are always placed in the direction above mentioned. If by following this indication of nature, we continue to detach the cryſtalline laminæ, we ſhall at laſt cauſe the form of the hexaedral priſm to diſappear totally, and in place of it, a rhomboidal parallelopiped will be obtained (fig. 2.), of which the plane angles at the rhombs will be 86° and 94°; the ſolid angles

angles at the ſummit * will meaſure 84° 31′; and that taken at the reunion of the baſis will be 95° 29′.

We can ſplit this parallelopiped only in a direction parallel to its faces; it will ſtill conſequently preſerve the ſame form, which is that of the nucleus of this ſubſtance, and its primitive form. It is, therefore, by a modification of the rhomboidal parallelopiped (fig. 2.) that nature has formed the regular hexaedral priſm (fig. 1.) which this ſubſtance preſents.

For if we conceive that in any period whatever of the increaſe of the rhomboidal parallelopiped, a ſeries of laminæ, or cryſtalline plates, has been depoſited on all the ſides of the parallelopiped; and that theſe laminæ have all undergone a progreſſive decreaſe of one row of cryſtalline molecules, at the acute angle which tends to form the ſummit; and alſo along the ſides of the oppoſite acute angle (fig. 3. and 4.); there will neceſſarily reſult from the continuation of this ſuperpoſition, to a certain period, an hexaedral priſm, terminated by two triedral pyramids, placed in a contrary direction; and their planes, or faces, which form a ſolid angle, of 147° 26′, with the ſides of the priſm, will be either pentagonal (fig. 3.) or triangular (fig. 4.). They will alſo have, in place of a ſummit, an equilateral triangular plane, ſometimes greater and ſometimes ſmaller.

If the ſuperpoſition continues, the equilateral triangular plane, on the ſummit, will become nonagonal, and there will remain no other traces of the primitive planes of the rhomboidal parallelopiped than ſmall iſoſceles triangular planes (fig. 5.): if the ſuperpoſition ſtill continues, until the laſt cryſtalline lamina is reduced to a ſingle molecule, or point, no appearance of the rhomboidal parallelopiped will then remain; and the cryſtal reſulting from this operation of nature will be a regular hexaedral priſm (fig. 1.).

In the ſame manner, viz. by a decreaſe on the lower edges of the laminæ, the primitive rhomboidal parallelopiped of calcareous ſpar paſſes to a regular hexaedral priſm of that ſubſtance, though more frequently it does ſo by a decreaſe on the lower angles of the laminæ. When the laminæ of the corundum cryſtal have, during their ſuperpoſition on the planes of the primitive rhomboidal parallelopiped, experienced a progreſſive decreaſe at one of their acute angles, and along the ſides of the other, at the ſame time, and in the ſame proportion, it is eaſy to conceive that the height of the hexaedral priſm muſt be the ſame as that of the rhomboidal parallelopiped, upon which it has been formed. The height B C (fig. 1.) muſt therefore bear the ſame proportion to the line A B, drawn through the middle of the two oppoſite ſides of the planes, on the extremities, as the whole height E F of the rhomboidal parallelopiped (fig. 2.) bears to the ſmall diagonal G H, from one of the rhombs, that is, nearly as 6, 45 : 5.

But although this exact proportion appears in a very great number of corundum cryſtals, yet we meet with ſome, whoſe lengths are more or leſs conſiderable; and this is owing

* For greater clearneſs, this rhomboidal parallelopiped may be conſidered as being formed by the junction of two triedral pyramids, baſe to baſe; and the two ſolid angles (each of which is formed by the reunion of three of the acute angles on the planes of the rhomb) will then be conſidered as the ſummits of theſe pyramids.

to

to different circumſtances, which have exiſted at the time of their cryſtallization. We may conceive, for inſtance, that if before the progreſſive decreaſe of the cryſtalline laminæ, in the manner above mentioned, the increaſe of the rhomboidal parallelopiped had taken place, by a ſuperpoſition of laminæ, in which the rows of cryſtalline molecules experienced a progreſſive decreaſe along the edges of the acute angle of the baſe only (fig. 6.), and that (the ſides of the priſm having already acquired a certain length) the ſucceeding cryſtalline laminæ had experienced a decreaſe at the acute angle of the ſummit, the ſame regular hexaedral priſm would have reſulted from this proceſs; but the proportion between the height, and the line drawn from two of the oppoſite ſides of the planes, on the extremities, would have been much greater than that of 6,45 : 5, and conſequently this priſm would have been longer than that of the rhomboidal parallelopiped, which ſerved as its nucleus. On the other hand, if the increaſe of the rhomboidal parallelopiped had taken place, by a ſuperpoſition of cryſtalline laminæ, decreaſing at the acute angle of the ſummit, and ſome time after decreaſing alſo along the ſides of the acute angle of the baſe (fig. 7.), the regular hexaedral priſm reſulting from this proceſs, would have been ſhorter, in proportion to the duration of the mode of decreaſe in the cryſtalline laminæ, which were firſt depoſited. There are ſome of the hexaedral priſms, in corundum cryſtals, which are ſo ſhort, that they appear no more than ſegments. Calcareous ſpar offers the ſame phenomenon; as do likewiſe all the ſubſtances in which the hexædral priſm has any analogy of formation, with that which we have here deſcribed.

It happens frequently, when the ſuperpoſition of the cryſtalline laminæ does not go on equally on all the faces of the rhomboidal parallelopiped, that one or two only of the ſolid angles of the hexædral priſm, taken alternately, ſtill ſhew, by ſmall iſoſceles triangular planes, ſome remains of the faces of the parallelopiped, while the others do not ſhew any at all.

Mr. Greville, in his collection of this ſubſtance, has a cryſtal of corundum, upon one ſide of which only two of the planes of the rhomb have experienced an equal and perfect ſuperpoſition, while there has been but a very ſmall number of cryſtalline laminæ depoſited on the third plane. Conſequently this cryſtal preſents a regular hexaedral priſm, one of whoſe ſolid angles is ſo much truncated, that the half of the plane of the end of the hexaedral priſms diſappears (fig. 8.); and this cut, or ſection, forms an angle of 122° 34′ with the plane on the extremity.

It is unneceſſary to obſerve, that the regularity of the hexaedral priſm depends on that of the rhomboidal parallelopiped, on which it is formed.

When, by detaching the laminæ from the alternate ſolid angles of the regular hexaedral priſm, the planes reſulting from this operation begin to run into one another; and the cryſtal begins to aſſume the form of the rhomboidal parallelopiped, to which it owes its origin; we frequently ſee the ſurface of theſe new planes divided into an immenſe number of ſmall rhombs, formed thereon by the interſection of lines, that are parallel to the ſides, which belong to the rhomboidal form of the new faces (fig. 9.).

Theſe lines are owing to the extremities of the laminæ, which have been depoſited on the inferior faces, correſponding with thoſe on which we obſerve them; and they ſerve to corroborate

roborate ſtill further, the demonſtration we have given of the formation of the regular hexaedral priſm in this ſubſtance.

(To be concluded in our next.)

IV.

On Water-wheels.

To Mr. NICHOLSON.

SIR,

YOUR attention to the inquiries ſent laſt month, induces me to tranſmit the annexed paper for your conſideration.

Your opinion that a float-board-wheel is preferable to a bucket, agrees with that of an experienced mill-wright.

I hope, through the medium of your valuable Journal, the ſubject will receive further diſcuſſion; as the ground on which the preference is given is not obvious.

It will, no doubt, be agreeable to you to learn, that the ſphere of your Journal's uſeful communication is extended to the northern verge of Ireland; and that, amidſt the tumult of political ſtrife, the arts of peace, and the intereſts of ſcience, are not yet forgotten.

I am, your obliged reader,

February 1798. DERRIENSIS.

On Water-wheels.

THE effect of water-wheels, ariſing from the gravitating power of the water; to retain the water, ſeems the point neceſſary to be attended to, in the conſtruction of the wheel.

The form of the bucket-wheel ſeems beſt adapted to this end; yet in ſtreams, where the fall is not conſiderable, a preference is given to float-board-wheels.

Why is this preference given to float-board-wheels? Suppoſe a ſtream which has a fall of ſix feet, and a ſupply which renders it equal to the power of ſix horſes:

Allow this power to be adequate to raiſe a given weight, one foot high, thirty times in a minute.

It is convenient to have a water-wheel of ten feet diameter. A bucket-wheel is made of a width adequate to the ſupply of water. Will this wheel be leſs competent to the work than a large float-board-wheel? If ſo, to what is the loſs of power in the ſmall, or the gain of power in the large, wheel owing?

WITHOUT entering minutely at preſent into the ſubject here offered for conſideration, which I leave to my other correſpondents, I ſhall only remark, that the preference ſeems to have been given to the cloſe breaſt-wheel, for ſtreams affording conſiderable quantities of water

water with a low fall, for the following reasons: 1. The load upon the bucket-wheel produces more friction, on the extremities of the axis, than is produced in the breast-wheel by the water, during its passage down the channel. That this reasoning is good, may be shewn by a very familiar experiment. Let any wheel be made to rest on its pivots, and then turned round; it will gradually lose its velocity, by the friction, to a certain point, when it will stop all at once. The suddenness of this stop will indicate the magnitude of the friction. If this be small, the velocity will much more gradually decay than in the contrary case. On the other hand, to shew the friction at the surface of a fluid, let a bowl be filled with water, and the fluid be stirred, so as to give it an horizontal motion; it will be found, that this large and heavy mass, bearing on a surface so very much exceeding that of the pivots of the wheel, will turn round for a considerable time, and lose its motion by very imperceptible gradations: whence it will follow, that the friction is much less. 2. In the usual construction of overshot-wheels, part of the fall is lost, in delivering the water, at a certain distance below the vertex, and part of its action is lost, by its running out of the buckets, before it has arrived at the lowest point of the revolution. 3. It is a general notion, that large wheels go steadier, from their operating in the manner of a fly, by means of their inertia; and wheels have been actually constructed of iron, with a view to this effect. The breast-wheel is always larger than the overshot-wheel, under like circumstances; and it may be constructed, under certain limitations, of as large a size as the engineer may think fit. 4. It is true, that a breast-wheel, of any size, might be converted into a bucket-wheel, and carry down the water in its buckets, instead of its being suffered to slide down the usual channel. But, when we imagine this conversion to take place, we see, at once, that the action would be less steady, from the successive filling of the buckets, and that the lower portion of the loaded periphery would discharge some of its contents much earlier than the point at which the breast-water stream would have ceased to act.

The numerical computation of the difference of effect between one wheel and the other, according to the dimensions given in the present communication, may be made from the data at page 466 of our present volume. It is there deduced, that one horse will raise two hogsheads and a half of water, ten feet high, in a minute; and this effect, from the note at page 465, would require the fall of twice that quantity of water, as a power to overcome friction, and produce velocity of working. The stream mentioned by my correspondent must, therefore, afford 415 cubic feet of water per minute, with a fall of 6 feet, if it be equal in power to six horses.

If certain dimensions be assumed for the buckets of the overshot-wheel, and an ordinary breast-wheel be compared with it, the quantity and fall of the water, uselessly expended on the former, will shew the difference of effect.

W. N.

On

V.

On the Glass Trundles of Citizen RENAUT; *and the Duration of the Teeth of Mill-work.* *By C. B.*

To Mr. NICHOLSON.

SIR,

I OBSERVE in your Journal for January last, an account of Citizen Molard's having substituted glass in the place of iron for the trundles of lantern pinions, which he states to be much preferable to the latter; as he says, cast-iron wears out the wooden teeth they act against in about four months; whereas, the glass, in the experiment he made, wore the wooden teeth about one-twelfth of an inch only in eighteen months.

The use of glass, in the trundles of lantern pinions, is liable to many objections; experience, however, may point out methods of obviating many of these: but I think it right to mention, that Citizen Molard is extremely wrong, when he says, cast-iron wears out the wooden teeth in four months; if it does so in France, it is otherwise in this country: I have had a mill of my own at work for these four years, in which wooden teeth act against iron, and I have never been obliged to get so much as a new tooth. Some months since I saw a steam engine, at Mess. Fish and Yates', in St. John-street, which turns machinery for his grinding of snuff and cutting tobacco. This machine was erected by Mr. Rennie, engineer of this city, in the year 1786; and has been constantly at work for about fifteen hours per day ever since; yet the wooden teeth, in the first motion, which act on cast-iron, had not worn above one-sixth of an inch; and the iron teeth did not seem to be worn $\frac{1}{50}$ part of an inch: and from every appearance, they will work without repair for ten or twelve years to come; as the workmen told me, that they had not, apparently, worn any thing for these six years past. They appeared to be very well executed; but not better than Mr. Rennie generally does such work.

By publishing the above, you may, perhaps, prevent an ill-founded prejudice from being taken against the use of cast-iron, in mill-work; which, from experience, I can say, is the best material I have known to be used.

I am, Sir,

Your most humble servant,

Feb. 4th, 1799. C. B.

I have visited the manufactory of Mess. Fish and Yates', and find that the statement given above is perfectly correct. It may be presumed, that the iron trundles, mentioned by Citizen Renaut, at page 522, were either ill finished, or rough from the forge; and, at all events, it appears, that they ought not to afford ground for any prejudice against good work in the usual materials.

W. N.

nO

VI.

On Dr. Parr's Theory of Light and Heat. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

ACCIDENT prevented my ſeeing, for ſome time, your Journal, publiſhed in December laſt; and I now take the liberty of making a few obſervations on ſome remarks of your correſpondent, reſpecting Dr. Parr's theory of light and heat*. He obſerves, "with reſpect to Dr. Parr's theory, that light and fire repel each other, when diſengaged from matter, it appears to me too fanciful to be ſolid, and too inconſiſtent to be ſupported," &c.—I do not quote the whole paſſage, becauſe I mean not to commence a controverſy upon the ſubject, as from the obſervation juſt tranſcribed, as well as from what follows, it is evident that your correſpondent has ſeen only the quotation in Dr. Pearſon's paper; and as the eſſay was publiſhed before the appearance of your Journal, I requeſt only room for a ſhort analyſis of the theory, that it may not be, as I ſuſpect it has been in the paſſage before me, miſunderſtood. I ſhall take up a little ſpace only in your valuable publication, but you will allow me to add, that M. Gadolin has ſince publiſhed an opinion not very different; and that, when Count Rumford ſpeaks of changes effected, not "by any chemical combination of the matter of light with ſuch bodies (as are expoſed to it), but merely by the heat which is *generated* or *excited* by the *light* which is *abſorbed by them*," he in reality uſes the ſame language.

Dr. Parr does not merely aſſert an opinion, but ſupports his hypotheſis by numerous and well-connected facts. Light he conſiders, with the chemical philoſopher of the preſent day, as a ſubſtance capable of combining with the various bodies it meets, producing by that means many chemical changes. Among theſe, he traces with peculiar care its influence upon vegetation; and from the decompoſition of water, afforded in the leaves of vegetables, ſeemingly by its power, he explains the great variety of gaſes expired by plants in different ſituations.

As light thus ſeparates oxygen, and as the latter probably contains heat in a leſs compounded ſtate than any other ſubſtance, he ſteps over the next difficulty, and ſuppoſes that it ſeparates heat. This is the only ſtep unſupported by immediate facts; but if aſſumed for a moment as an hypotheſis, the concurrence of numerous other facts will raiſe it to a higher rank. By what means light and heat are brought together in union, when they are ingredients in inflammable bodies, he pretends not to explain; but if they are really different elements, as is now generally believed, our not knowing the bond of union, is no proof againſt its exiſtence. In the examination of the various exploſive ſubſtances, Dr. Parr traces in each the ſource of its light and heat, and ſhows by what means the balance ſubſiſting between theſe active ingredients is deſtroyed, ſometimes by the addition of the one, ſometimes by that of the other principle. There are various exploſive ſubſtances which will not explode

* Eſſay upon Light, in a miſcellaneous volume, publiſhed by a ſociety at Exeter.

plode without light, however intense the heat, and *vice versâ.*—This part of the essay, where he examines the source of light in the composition of the body, and explains the different modes by which the violent separation is effected, as well as the consequences of the explosion, appears highly ingenious.

Such, Sir, is the outline of the paper which I think is misunderstood and misrepresented by your correspondent. The different parts can be only fairly appreciated by an examination of the facts. This is a business to which I cannot now attend, and which is not suited to your Journal. Your giving me a little room for this analysis will oblige, Sir,

Yours,

A Constant Reader.

SCIENTIFIC NEWS AND ACCOUNTS OF BOOKS.

Institute of Cairo.

(Continued from page 523, vol. II.)

THE second sitting was held on the 11th Fructidor, at seven in the morning, at which Citizen Andreossy made a report on the fifth question proposed by Buonaparte. He observed, in the first place, that Egypt possesses no sulphur, but formerly imported it from Venice. The charcoal is obtained by burning the lupine in a trench, and afterwards sifting it. The saltpetre is native, and is even said to be found in veins *(par veines)* round Cairo. It is refined in the same manner as (commonly) in Europe. It is a true nitrate of potash, and not of lime, as in France. The boiling is made with the stalks of Turkey-wheat, and it is purified with whites of eggs. The gunpowder is made by hand, and the workmen are naked. Each mortar contains fifteen pounds, and the process lasts seven hours. The pestles weigh from nineteen to twenty-five paros. A small quantity of water is added, and the granulation is performed by a sieve. The reporter affirms that *this powder is very good*; but that to render it still better it is necessary that the proportions of France should be followed. Two thousand cantars (were) formerly made at Cairo, of which much was sent to Leghorn. The beys had very little powder. Murat Bey had no more than 1,500 cantars. It would be easy, says the reporter, to augment this fabrication, and even to export large quantities to Europe.

At the same sitting, the citizen Monge read a memoir on the phenomenon, called *le mirage* by French seamen, and by ours *looming*. This effect of refraction was remarkably seen by the French, in the course of their march through the desert. Villages seen at a distance appeared elevated, and as if built on an island in the middle of a lake. The surface of this apparent water became narrower as they approached, until at length, when they were only at a small distance, it disappeared; but the same illusion presented itself, with regard to the next

next remote village. The author afcribes this phenomenon to a diminution of the denfity of the lower ftratum of the atmofphere, occafioned by evaporation of water from the fands. It is impoffible to deduce the particulars of his theory from the mere report; but it feems to be nearly the fame as that of Mr. Huddart*.

The third fitting was held on the 16th Fructidor. Berthollet read a memoir on the formation of ammoniac. Citizen Sulkowfky read a defcription of the route from Cairo to Salehia. Egypt has hitherto been known only on the banks of the Nile. The road followed by the French army in purfuit of Ibrahim Bey was entirely unknown. On going out of Cairo by the gate of Nafr, you enter the defert, where feveral forfaken houfes were obferved. The village of Elmaria, on this road, is the ancient Heliopolis. In another village, called Elmaria, there were thoufands of palm-trees. On the right of this road, there are nothing but deferts of fand; but, on the left, are many cultivated lands. The places which the army paffed are Lacoubey, Elhanea, Elmenia, Belbeys, Souva, Coraim, Salehia, and many others, which it merely paffed through in hafte.

Citizen Say fpoke of the methods of obtaining better means of grinding, and fhewed that water-mills ought to be conftructed.

C. Balholet (q. Berthollet?) read a memoir, in which he examines the gun-powder of Cairo. It contains only five parts out of thirty-two of falt-petre; the reft is fulphur, charcoal, earthy matter, and muriate of foda: fo that *the only means of rendering it ufeful, is to wafh out the falt, and manufacture it a fecond time.*

Citizen Monge read a memoir on the antient monuments of Cairo. The fafar, in the ftreet which leads from the inftitute to the caftle, prefents a vafe of granite with hieroglyphics within and without. It was a tomb, and refembles that of the mofque of Alexandria. Citizen Monge propofes that it be removed by the Inftitute, in order to convey it to the mufeum of France. Near the caftle, after having paffed the palace of Jofeph, there is the cell of a gate with hieroglyphics well preferved.

The fourth fitting, 21ft Fructidor. Citizen Sulkowfky read a memoir on a buft of Ifis. Say, another on the materials for fuel in Egypt. The commiffion for grinding, announced their intention of erecting a wind-mill. Citizen Geoffroy read a memoir on the oftrich.

The fifth fitting, 26th Fructidor. The commiffion for fuel, reported that the ftems of carthamus, reeds, and ftraw, may be ufed for heating ovens, at lefs charge than in France.—Buonaparte prefented the *Connoiffance des Temps* for the year VII, and invited the Inftitute to compofe an almanack. A commiffion was appointed for this object, confifting of Monge Beauchamp, Nouette, and Raphael. It was at the fame time decreed, that the three following almanacks fhould be united, namely, that of the Copts, the Muffulmen, and the French. Citizen Fourier read a memoir on the folution of algebraic equations. Citizen Parceval, a tranflation of a fragment of Taffo. Citizen Defgenettes read a differtation on the diarrhœa, the dyfentery, and the endemic opthalmia, which are more peculiarly the diforders of Egypt.

* Philofophical Journal, I. 145.——This curious fubject, which does not yet feem to be adequately explained in all its particulars, has employed the meditations of various obfervers; for fome of which, the reader may refer to the authors quoted at page 418 of our prefent volume.—N.

The

The ſixth ſitting, on the firſt complementary day. Citizen Beauchamp preſented an annuary, ready for immediate printing, and alſo ſeveral aſtronomical obſervations. Citizen Berthollet read a letter from Citizen Laplace, announcing a correction in the metre. He alſo gave an account of the manufacture of indigo, and pointed out conſiderable improvements. Citizen Fourier communicated a project of a wind-mill, to water lands.

Sixth ſitting, on the 6th Vendemaire, year VII. Citizen Pouſſielque preſented a ſketch of a new method of analytically demonſtrating geometrical theorems, by Corancez the younger. Citizen Norry read a memoir on Pompey's pillar. Savigny deſcribed a new ſpecies of nymphæa. A commiſſion was charged with the eſtabliſhment of a ſchool of deſign, among whom were Redouté and Norry. Citizen Coſtaz read a memoir on the colours of the ſea, and Citizen Parceval another tranſlation from Taſſo.

Eighth ſitting, Vendemaire, year VII. Fifty mummies of birds, ſent to the Inſtitute, were delivered for examination to a commiſſion, compoſed of Buonaparte, Geoffroy, Dolomieu, and ſome others. Porte, a native of France, and inhabitant of Cairo, who is employed on Indigo, preſented ſamples to the Inſtitute. Citizen Larrey read a memoir on the opthalmia. Citizen Beauchamp read another on his voyage to Trebiſond. He indicates the longitude of Iſpahan, and obſerves, that the longitude of Trebiſond is 37° 18′ 5″ from Paris, and not 43°, as Bonne affirms; which deducts more than eighty leagues from the Black Sea. Citizen Deliſle read a memoir on the palm-tree, which bears the fruit called domm. It is the caſſiophora of Theophraſtus. Citizen Dolomieu read a memoir on the ſtudy of ancient and modern geography. He fixes the poſition of the ancient Alexandria between two hills of calcareous ſand-ſtone, and explained the ſubſequent changes. He thinks the ſea muſt have riſen a foot ſince the time of the Ptolemies. Citizen Norry made a report concerning the ſchool of deſign; and Citizen Parceval read a tranſlation from Taſſo.

Since the commencement of the VIIth year, a literary journal has appeared, under the title of *Decade Egyptienne, Journal litteraire et d'Economie politique.* The proſpectus was ſigned *Tallien*, and is compoſed of thirty-eight pages. The Journal is to appear every decade, each number containing two and a half or three ſheets, in octavo. The price per number is to be 1 franc French money, or 10 francs per 12 numbers. Subſcriptions are taken by Citizen Marc-Aurell, printer to the army, quartier des François, at Cairo. The firſt number appeared the 10 Vendermaire, year VII.

An Account of the Operations carried on for accompliſhing a Trigonometrical Survey of England and Wales, from the Commencement in 1784, to the End of the Year 1796. Begun under the Direction of the Royal Society, and continued by Order of the Honourable Board of Ordnance. Firſt publiſhed in, and now received from, the Philoſophical Tranſactions. By Captain William Mudge, F.R.S. and Mr. Iſaac Dalby, Vol. I. Illuſtrated with 22 Copper-plates. London, printed by Bulmer, and Co. for Faden, Charing-Croſs, 1799; 437 pages, Price 1l. 8s. boards.

ALL Europe is acquainted with more or leſs of the particulars of this ſurvey, which is a work of the utmoſt utility, and an honour to our government, and the parties to whom the execution

execution has been intrusted. The accounts having hitherto been published only in the Philosophical Transactions, must necessarily have been of confined circulation, while the number and expence of the engravings have prevented any satisfactory detail from appearing in other publications. The geographer and man of science will, consequently, learn with great pleasure, that the account of the operations, as far as they have yet been carried, together with the plates of instruments, surveys, and other results, are to be had, in a beautifully printed quarto volume, at a very easy price.

The original design had, for its immediate object, the ascertaining of data, by which the difference in longitude, between the observations of Greenwich and Paris, might be determined. Soon after the death of General Roy, the general survey of the kingdom was commenced, of which accounts were published, in the Transactions for 1796. And in 1798, Mr. Faden having determined on republishing all the papers relative to this object, the president and council of the Royal Society rendered him the very essential service of furnishing him with the original copper-plates, and the master-general of the ordnance granted him permission to reprint the accounts of the subsequent trigonometrical survey. This is now done under the superintendence of the able men whose names appear in the title page, who have omitted no care to render this collection of equal value with the originals, and in some few respects more so. An account of the changes, which the editors have made in the original papers, is given in the preface; where we also observe, that Mr. Faden, with the permission of the Board of Ordnance, intends to publish a very superior map of Kent, in the course of the present year, from documents supplied by the labours of the gentlemen employed on the general survey by government.

A public institution for diffusing the knowledge and facilitating the general introduction of useful mechanical improvements, and for teaching by courses of lectures and experiments the application of science to the common purposes of life, has been proposed to be established, by subscription, in London, by some gentlemen of the first respectability. I have seen a printed paper, containing the outlines of a plan, and a list of gentlemen, who have subscribed fifty guineas each, for this purpose. As the plan itself is still under deliberation, with regard to various essential particulars, and the present list of subscribers does not contain the whole of the names, on account of the rapidity with which that list increases, I shall enter into no further detail at present, than simply to observe, that the plan appears to have already met with a degree of support, which is no less honourable to the public spirit of the subscribers, than distinctive of its own value and importance.

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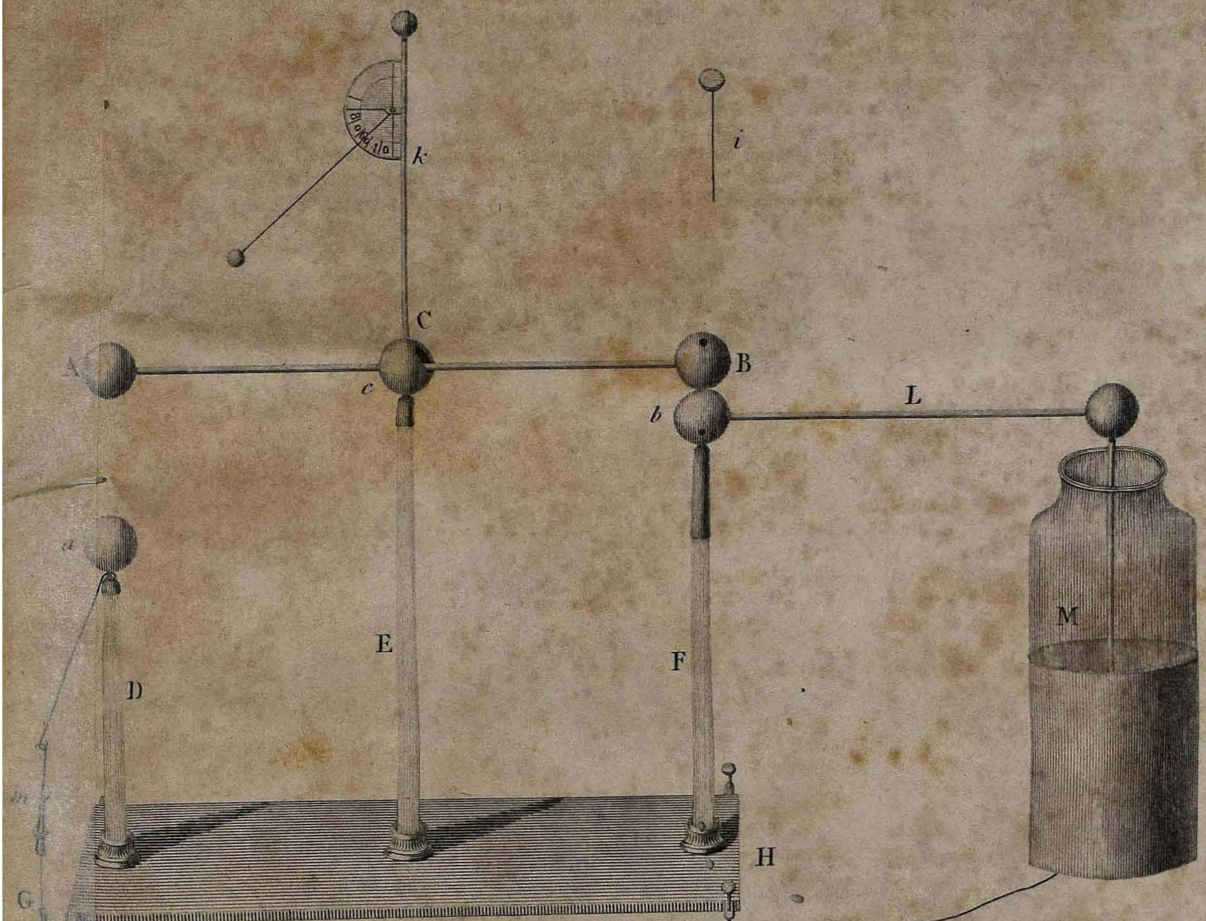

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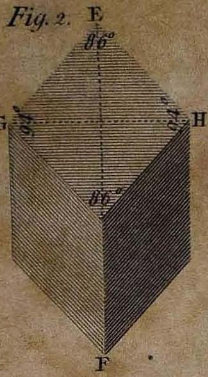

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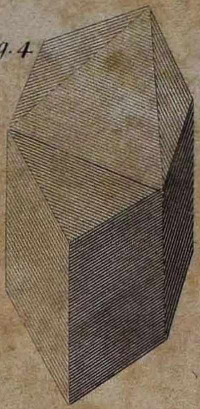

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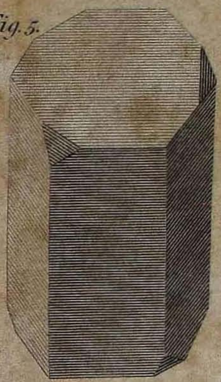

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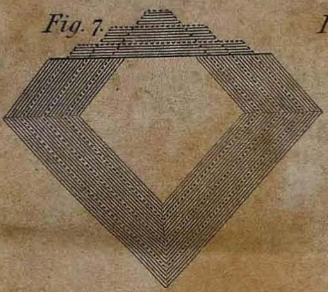

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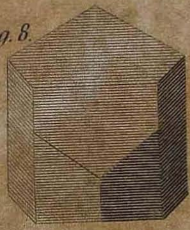

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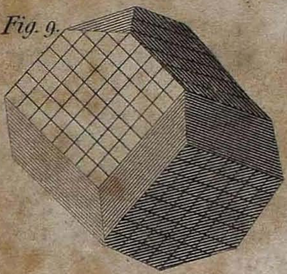

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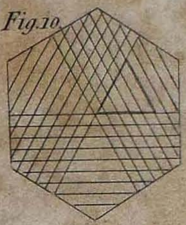

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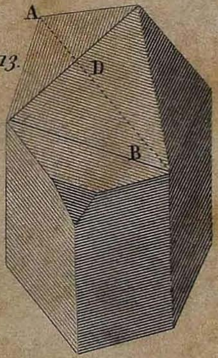

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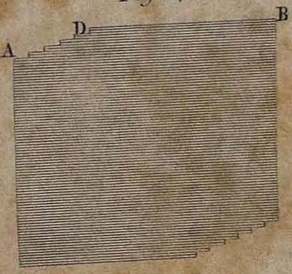

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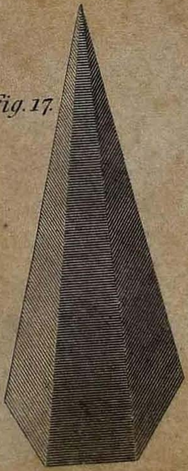

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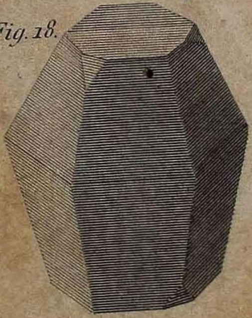

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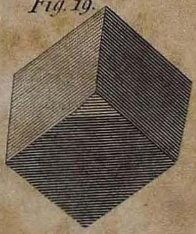

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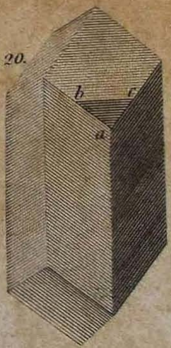

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ERRATA IN VOL. II.

Page.	Line.	
29	3	from the bottom, for *as*, read *aa*
46	13	after *page* 424, infert *Vol. I.* The plate of *Apparatus* fhould be numbered II.
122	18	for *periodical*, read *fynodic*
	19	for 1 *d.* 8 *h.* 26 $\frac{6}{10}$ *m.* read 1 *d.* 18 *h.* 28 *m.* 36 *s.*
124	37	for *not quite* + 37″, read *upwards of* — 30″
—	40	for 97½ *day's ufe*, read 117 *and* 97½ *day's ufe refpectively*
—	*ult.*	for × read *of.*
125	11	What is faid of *the firft and fecond fatellites*, refpecting the annual error, is exactly true, only with refpect to *the fecond*:—the error of *the firft* being *minus*, and in a fmaller proportion.
264	—	In fome copies, the figures *in the note* have flipped, and give an imperfect impreffion. Read *the amount of Gold coined between the years* 1762 *and* 1772 *both inclufive, was* 8,157,203*l.* 15*s.* 6*d. and between* 1782 *and* 1792, *both inclufive, was* 19,675,666*l.* 14*s.* 6*d. and between* 1773 *and* 1777, *both inclufive, was* 19,591,833*l.*
		15. And in the fourth li[...] the bottom, read *recko*[...] *(half) per cent.*
275	8	read *the pipe* A B F E
439	8	from the bottom for *ftate* r[...]
488	14	from the bottom, erafe the [...] after the word *polifhed*, and [...] a full ftop after *length*
490	7	for *offered*, read *afforded*
—	3	from the bottom for K B r[...]
491	12	read *interior* part of the tu[...]
—	3	from the bottom for D D [...] D B F C
558	6	for *Biringuccius* read *Bering*[...]